

INTER-AMERICAN TROPICAL TUNA COMMISSION COMISION INTERAMERICANA DEL ATUN TROPICAL

Scripps Institution of Oceanography, 8604 La Jolla Shores Drive, La Jolla, CA 92037-1508
Tel: (619) 546-7100 - Fax: (619) 546-7133 - Director: James Joseph, Ph.D.

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Dr. Michael F. Tillman
Science Director
Southwest Fisheries Science Center
P.O. Box 271
La Jolla, California 92038-0271

Dear Dr. Tillman:

This letter is in response to Dr. Goodman's report which you sent to Dr. Joseph on February 1, 1999. The report does not acknowledge the points concerning an earlier draft, which were made in Dr. Joseph's letter of January 14. The essence of consultation is consideration of the issues raised, and I believe that the process should include a discussion of any suggestions which have not been incorporated. Thus I am not sure that the comments we made have been addressed, and I refer them to Dr. Goodman again. Also, I will go over some of the previously-discussed ground in this letter.

We have received a reply from Dr. Feidler in which he provides a response to suggestions made in my letter of February 3. I may provide further comment on the issues that he raises after having an opportunity to consider them.

Turning to the report, I will comment first on specific parts, and then make some more general observations based on recent relevant research of the IATTC staff.

In the third paragraph of page 1 of the report Dr. Goodman states that the northeastern offshore spotted population has apparently not recovered as expected, following the drop in the incidental mortality, based on the view that the relative abundance of the population estimated by the TVOD data "do not show a recovery." This statement does not take into account the precision and accuracy of those estimates. In fact, our exploratory or strategic analysis, referred to later, suggests that the estimates are consistent with a population growth rate of between 2% and 4% during the time period of relevance to this issue (1993-1998). In addition, at least the preliminary estimates of population abundance for the depleted stocks indicate that those populations have been growing,

On page 4, in the first paragraph of the section dealing with relative abundance, Dr. Goodman said that "... the TVOD indices agreed in general with research vessel data, except for an aberration in the 1983 estimates, so it is not unrealistic to expect that the IATTC's trend estimates reflect actual trends in dolphin abundance." The annual MOPS estimates for northeastern spotted dolphins range from 318,200 to 1,445,000, and the differences among them are several

times larger than their standard errors. Thus it is unrealistic to expect their variation to reflect population changes. Similarly, the TVOD indices for that period, and over their entire range, show annual variations, *e.g.* that between the indices for 1983 and 1985 and their neighboring years, which are too large to reflect population changes. Given this, I find it hard to see how any agreement in general during the period of the MOPS survey could provide an assurance concerning long-term trends in dolphin abundance.

I am surprised to read the comment in the first complete paragraph on page 5. I was not aware that Peter Perkins is "... reviewing the general statistical methodology that converts raw tuna vessel observer data into the indices of population abundance" without some of the essential details of the calculation. The Commission staff would be happy to assist him.

In the last paragraph of page 5 there is a reference to the relatively small (sample) variances for the TVOD estimates. I referred previously to differences between neighboring TVOD estimates which are too large to be credible, given the "small variances," indicating that the possibility of a process error ought also be taken into account in making comparisons with them.

In relation to the third paragraph on page 6 concerning the analysis of whether a regime shift has occurred, it is worth pointing out that the recruitment levels of yellowfin tuna have shown marked changes over the last 10 to 15 years, as described in the IATTC annual reports. It is plausible that similar unexpected changes have taken place for dolphin populations.

The first paragraph on page 7 states that the stress literature review will "... conclude whether stress ... **is or is not** (*emphasis added*) a plausible source of mortality and ...". I noted in my letter of February 3 that, in fact, the review appeared to deal only with the question of whether it was plausible that ... stress could have a population-level affect, and that a similar approach directed at the question of whether it was plausible that stress did not have an effect at the population level might well also reach an affirmative conclusion.

The first complete paragraph on page 9 postulates that "... the differences between the estimates of growth rates for the two periods may be interpreted as changes in cryptic kill, reproductive effects of chase and encirclement, or a misreporting or mis-estimation of kill." Most of the factors likely to be associated with any of those possible reasons for any reduction in growth rate would have a far greater effect in earlier periods because there were more chases and more sets on dolphin schools and because the mortalities per set were greater due to the less effective techniques employed for removing them from the net. The earlier years were probably associated with greater levels of stress, making the whole construction unlikely. Thus, I think that any change in growth rate between the two periods would more likely be due to vagaries in the way the estimates come out over a short time period.

The third paragraph on page 11 proposes expressing (any) reduction in population growth rate after 1991 as unreported additional mortality. While this may be a simple numerical convenience, it invites an unjustified interpretation of the data. If there is a depression of the growth rate, there is no evidence to show that is due to mortality, rather than to, say, a change in carrying capacity. Use of this device could lead to unwarranted conclusions.

Finally, on page 12 the propositions for evaluating potential adverse impact that Dr Joseph commented adversely on in his letter of January 14 are rephrased. To repeat the earlier objections, in two cases the criteria proposed amount to the question of whether there is a slight chance that there has been an adverse impact. Given the high level of variance and process error associated with the estimates, it is hard not to satisfy a test such as that. It might be instructive to calculate what rate of population growth would be necessary if the test was not to be satisfied. Such a conservative type of approach might be defensible in a question of resource management, particularly if a population was apparently in a precarious situation. However, the research is directed at the question of product labeling, rather than resource management. The criteria are clearly not appropriate.

In addition to comments on the report, I want to let you know of some work the IATTC staff have been doing in respect of the TVOD indices and strategic modeling to illuminate some of the issues involved in this complex situation.

In his letter of January 14, Dr Joseph warned that the TVOD indices should not be used to make comparisons between the early years and those after 1992. The work upon which that caution was based is still in progress, and there are two things which are of particular concern. First, the empirical distributions of the perpendicular distances for the early years of the series show an excess of sightings on the track line, suggesting that the vessels may have turned toward the herds of dolphins before the observer recorded the angles from the track line. This excess largely disappears in the late 1980s, possibly as a result of improved training. On the other hand, beginning in about 1992, the empirical distributions of the perpendicular distances often show a maximum in the number of sightings between 1.0 and 2.0 nm off the track line. These changes over time may lead to changes in the bias of the relative abundance indices, and thus affect the estimated trends. Second, the estimates of relative abundance of northeastern spotted dolphins are strongly correlated with the estimated numbers of dolphin sets made, raising the question of whether the estimates of relative abundance are unduly influenced by fishing effort. The correlation appears to be the result of a strong correlation between the estimated number of dolphin sets and the estimated encounter rate, and thus may reflect underreporting of sightings of dolphin herds by crewmen when they are not planning on making sets on tuna associated with dolphins. We are currently conducting research to improve those estimates. We have made progress, but more work needs to be done on the procedure.

The strategic modeling that we have does not purport to be a complete population model, but it raises some issues that ought to be addressed in the NMFS modeling.

First, it shows the importance of using the data on variation in the age composition of the dolphins which were killed in the fishery. I understand that the analysis used by Dr. Wade relies on a fixed age composition, which reflects the situation in some years. Estimates of age composition are available for only some years, so the strategic model for northeastern spotted dolphins has been based on color stages that are routinely identified by observers. Second, the effect of using fishing effort as an explanatory variable in the model should be taken into account. This improves the fit dramatically, and leads to different conclusions about the growth rate of the population. Third, the effect of fitting all the yearly estimates of previous surveys (taking account of their sample variances) instead of just using the pooled 1986-1990 estimate for the MOPS surveys, should be considered. Doing this has a marked effect on the estimates of population size for northeastern spotted dolphins, which suggests that the pooling approach was not only unsuccessful at achieving variance reduction, but produced a different population

estimate than the one obtained by the minimum-variance weighted mean.

I emphasize that both of these studies are still in progress, and we have no firm conclusions. However, the first study makes it clear that the comparisons between recent and later years of the TVOD data are not sound, and the second indicates that the modeling results may be sensitive to issues which, I think, have not been considered to date. Your staff may follow this up by personal contacts with Dr. Deriso or Ms. Lennert.

In summary, I believe the proposed decision analysis is so flawed that I recommend the NMFS abandon it and simply present to the Secretary of Commerce the best scientific results coming from the surveys, interpretation of the TVOD indices, modeling, and a review of stress literature, with appropriate comments about their reliability, and allow the Secretary to make his own decision about what would constitute an adverse impact and whether it has occurred.

Yours sincerely,

A handwritten signature in black ink, appearing to read "Robin Allen", with a long, sweeping horizontal line extending to the right.

Robin Allen
Assistant Director

CC: Commissioners